Striking Out? The Economic Impact of Major League Baseball Work Stoppages on Host Communities

Victor A. Matheson
and
Robert A. Baade

April 2005

COLLEGE OF THE HOLY CROSS, DEPARTMENT OF ECONOMICS
FACULTY RESEARCH SERIES, WORKING PAPER NO. 05-07[*]

Department of Economics
College of the Holy Cross
Box 45A
Worcester, Massachusetts 01610
(508) 793-3362 (phone)
(508) 793-3710 (fax)
http://www.holycross.edu/departments/economics/website

[*]All papers in the Holy Cross Working Paper Series should be considered draft versions subject to future revision. Comments and suggestions are welcome.

Striking Out? The Economic Impact of Major League Baseball Work Stoppages on Host Communities

Victor A. Matheson[†]
College of the Holy Cross

and

Robert A. Baade[††]
Lake Forest College

April 2005

Abstract

Major League Baseball teams have used the lure of economic riches as an incentive for cities to construct new stadiums at considerable public expense. Estimates of the economic impact of a MLB on host communities have typically been in the vicinity of $300 million. Our analysis suggest these numbers are wildly inflated. Using the baseball strikes of 1981, 1994, and 1995 as test cases, we find the net economic impact for a MLB team on a host city of $16.2 million under one model and $132.3 million under a second model.

JEL Classification Codes: L83

Keywords: impact analysis, baseball, sports economics

[†]Victor A. Matheson, Department of Economics, Box 157A, College of the Holy Cross, Worcester, MA 01610-2395, 508-793-2649 (phone), 508-793-3710 (fax), vmatheso@holycross.edu

[††]Robert A. Baade, Department of Economics and Business, Lake Forest College, Lake Forest, IL 60045, 847-735-5136 (phone), 847-735-6193 (fax), baade@lfc.edu

Introduction

Professional baseball is big business in the United States. Major League Baseball (MLB) attracts over 70 million fans to games each year with television viewing audiences many times this number. Top players routinely receive contracts that pay them in excess of $10 million per year. The construction of Camden Yards in 1992 has prompted a boom in stadium construction that has seen 14 new stadiums being completed during the 1992-2002 period with new stadiums being proposed for another 11 cities (Munsey and Suppes, 2000). The majority of the cost of these construction projects has been bourne by local taxpayers.

Many attempts have been made to quantify the economic impact that the presence of a MLB team has on a host city. Interest groups wishing to develop public backing for attracting a new team to a metropolitan area or existing Major League teams who wish to cultivate voter support for public financing of new stadium facilities frequently commission economic impact studies that report large contributions from professional sports teams to local economies. For example, the Oregon Baseball Campaign, a group dedicated to bringing MLB to Portland, reported that “a MLB team and ballpark would generate between $170 and $300 million annually in gross expenditures to the state of Oregon.” (Oregon Baseball Campaign, 2002) A similar study completed for the Virginia Baseball Authority stated that a “a major league baseball franchise and stadium in northern Virginia would pump more that $8.6 billion into the economy over 30 years,” or $287 million annually. The St. Louis Regional Chamber and Growth Association estimated that the Cardinals brought $301 million in economic benefits to the region with another potential $40 to $48 million in benefits from a post-season appearance. (St. Louis Regional Chamber and Growth Association, 2000) The lowly Montreal Expos, with attendance less than one-third that of the typical MLB team, produce "a GDP of $105.3 million" according to a federal report on sport in Canada. (Beaudry, 2002) Even pre-season games apparently generate significant economic activity. The Florida Sports Foundation announced that the Grapefruit League, the Spring Training league for 20 of the 30 MLB teams, generated a $490 million impact to the state from 306 games attracting just over 1.5 million fans in 2002. This combined attendance figure represents an amount roughly two-thirds that which the average MLB team attracts during the regular season.

Public finance economists, on the other hand, are in general agreement that the figures produced by sports boosters are wildly inflated. Many studies, including Baade and Dye (1990), Rosentraub (1994), Baade (1996), Noll and Zimbalist (1997), and Coates and Humphreys (1999) to name just a few, have examined the economic impact of stadium construction. Without exception, these studies have found that new stadiums provide little or no net economic stimulus to the communities in which they are located. Others such as Porter (1999) and Baade and Matheson (2000) have examined sports "mega-events" such as the Super Bowl and the MLB All-Star Game. Porter used regression analysis to determine that the economic impact of the Super Bowl on the host city was statistically insignificant, that is not measurably different from zero (Porter, 1999). Likewise, Baade and Matheson (2000) challenged an MLB claim that annual All-Star Game contributes $75 million to the host city economy. Their study of taxable sales data and employment data concluded that the All-Star Game was actually associated with lower than expected economic activity for host cities. Again, these researchers find that boosters' estimates of the economic impact of large sporting events exaggerate the true economic impact of these events by up to a factor of ten.

But the question remains, how big is the true economic impact of professional sports teams on their host cities? In particular, this paper will examine Major League Baseball teams' economic contribution to their local metropolitan areas using the 1981 and 1994/95 baseball strikes as test cases.

Review Sports Economic Impact Studies

The economic numbers quoted by baseball promoters are usually generated using a standard expenditure approach to estimating the direct economic impact of the event. The numbers are derived by estimating the number of "visitor days" as a result of the team and multiplying that statistic by the average estimated per diem expenditures per visitor. Once an estimate of direct impact is obtained, the total economic impact is estimated by applying a multiplier which typically doubles the direct economic impact. Using this technique, if a mistake is made in estimating direct expenditures, those errors are compounded in estimating indirect expenditures. The secret to generating credible economic impact estimates using the expenditure approach is to accurately estimate direct expenditures. A annual figure of $300 million for a MLB team could be roughly arrived at simply by assuming 2.5 million fans per year (only slightly more than the MLB average) each spending an average of $60 on their visit and then applying an economic multiplier of 2.

Precisely measuring changes in direct expenditures is fraught with difficulties, however. Most prominent among them is an assessment of the extent to which spending in conjunction with the event would have occurred in the absence of it. For example, if an estimate was sought on the impact of a professional sports team on a local economy, consideration would have to be given to the fact that spending on the team may well merely substitute for spending that would occur on something else in the local economy in the absence of the event. As pointed out by Andrew Zimbalist when discussing the 2000 "Subway Series" between the New York Mets and New York Yankees, "If you buy a $100 ticket to the Series, that's money you might have spent on a Broadway show or food." Therefore, if the fans are primarily indigenous to the community, a MLB team may simply yield a reallocation of leisure spending while leaving total spending unchanged. This distinction between gross and net spending has been cited by economists as a chief reason why professional sports in general do not seem to contribute as much to metropolitan economies as boosters claim (Baade, 1996). There is nearly universal agreement among independent economists that spending by local residents must be excluded from economic impact calculations due to this substitution effect.

For example, only $37.9 of the reported $105.3 million estimated impact of the Montreal Expos comes from outside of Montreal. Similarly, the St. Louis Cardinals report that only 32% of their fan base comes from outside the St. Louis metropolitan area and that percentage is one of the highest in the league. (St. Louis Regional Chamber and Growth Association, 2000) The large economic impact figure for the Grapefruit League is largely a result of the fact that spending on this league is more likely to be categorized as export spending since most of it is thought to be undertaken by people from outside the communities with 60% of spring training fans being visitors to the Florida region.

At first blush, excluding local spending from economic impact analyses should eliminate the upward bias in the calculations. An examination of mega-events, which tend to have an even larger percentage of visitors coming from outside the host region (87% of attendees at the 1999

Super Bowl were from outside the host city of Miami), still shows that booster claims are far above *ex post* analyses of the actual economic effects of these events.

Spending by local residents, therefore, is not the only potentially significant source of bias in estimating direct expenditures. While surveys on expenditures by those attending a sporting event complete with a question on place of residence, may well provide insight on spending behavior for those patronizing the event, such a technique offers no data on changes in spending by residents not attending the event. It is conceivable that some residents may dramatically change their spending during an event in order to avoid the congestion in the venue's environs. Similarly, while hotel rooms during a local team's home stand may be filled with baseball fans, if hotels in the host city are normally at or near capacity during the time period in which the team is playing at home, it may be that sporting event visitors are simply crowding out other potential visitors. In general, a fundamental shortcoming of economic impact studies is not with information on spending for those who are included in a direct expenditure survey, but rather with the lack of information on the spending behavior for those who are not.

Even out of town visitors who attend a sporting event may not improve the local economy if their attendance at the game displaces other activities in the city that the visitors would have done instead. For example, each year at the Western Economic Association meetings, the conference organizes a baseball outing for attendees. While these visitors to the conference city undoubtably spend money at the baseball game, these same visitors would have spent money going out to dinner or to another cultural attraction in the absence of the baseball game. In other words, even though the sports franchise induces visitor spending, it does not induce any new spending in the city. In this case, the baseball team does not add economic activity to the city but simply reallocates spending from one area to another.

A second potentially significant source of bias in economic impact studies relates to leakages from the circular flow of spending. For example, if the host economy is at or very near full employment or if the work requires specialized skills, it may be that the labor essential to conducting the event resides in other communities. To the extent that this is true, then the indirect spending that constitutes the multiplier effect must be adjusted to reflect this leakage of income and subsequent spending.

Labor is not the only factor of production that may repatriate income. Even if hotels experience higher than normal occupancy rates during a sporting events, then the question must be raised about the fraction of increased earnings that remain in the community if the hotel is a nationally owned chain. In short, to assess the impact of mega-events, a balance of payments approach must be utilized. Since the input-output models used in even the most sophisticated *ex ante* analyses are based on fixed relationships between inputs and outputs, such models do not account for the expenditure complications associated with full employment and capital ownership noted here.

As an alternative to estimating the change in expenditures and associated changes in economic activity, those who provide goods and services directly in accommodating the event could be asked how their activity has been altered by the event. Unfortunately, most business managers are unable to accurately predict how much economic activity would have taken place without the event.

Since the expenditure approach to projecting the economic impact of mega-events is most commonly used by league and city officials to generate economic impact estimates, we will be comparing the results generated by our model to the estimates quoted by league officials that were derived using an expenditure approach. In the next sections of the paper, the models that are used to estimate the impact of the MLB are detailed.

Model #1

The economic activity generated by a MLB team is likely to be small relative to the overall economy, and isolating the team's impact, therefore, is not a trivial task. The largest economic estimates of $300 million represent only 0.6% of the personal income of even the smallest MLB markets (Fort Worth, Kansas City, Milwaukee, and Cincinnati) and only about 0.1% of the largest MLB markets (New York City, Los Angeles, and Chicago).

An additional difficulty posed by MLB is the fact that its anti-trust exemption has prevented significant movement in teams. The last team to relocate was the Texas Rangers who moved from Washington, D.C. in 1971. This provides few opportunities to examine the economic impact of the loss of a team. Several teams have been added through expansion in the past 30 years: the Seattle Mariners and Toronto Blue Bays in 1977, the Colorado Rockies and Florida (Miami) Marlins in 1993, and the Tampa Bay Devil Rays and Arizona Diamondbacks in 1998. While it is certainly possible to examine these cities to attempt to estimate the effect of adding a MLB team, one may run into an endogenous variable problem in the analysis. Certainly MLB bases its decision on which cities to grant expansion franchises at least in part on the economic growth prospects of the applicant cities. Therefore, the question arises: if a city experiences rapid economic growth following being granted an expansion franchise, is the growth the result of the franchise or was the franchise granted because of good prospects for economic growth in the city?

With these problems inherent in estimating the economic effects of a MLB franchise in a metropolitan area, it is natural to search for other ways to measure these effects. The 1981 and 1994 MLB strikes provide natural experiments. The 1994 players' strike resulted in the cancellation of 669 regular season games (29.5% of the total season) as well as the entire post-season including the World Series. The strike also resulted in the loss of 252 regular season games in the 1995 season (9 of 81 normally scheduled home games per team). In addition, the ill-will prevailing among baseball fans that resulted from the 1994 strike and the cancellation of previous year's World Series caused a 29% reduction in overall baseball attendance in 1995 compared with the non-strike year of 1993. The 1981 strike resulted in the loss of 717 regular season games (34% of the total), but the strike was resolved before the post-season so these games were not lost in 1981. Perhaps due to this fact, MLB suffered no let down in attendance in the following year unlike after the 1994 strike. Eighty-six regular season games were also lost due to a work stoppage in the 1972 season. The total number of games lost in 1972 represents less than 5% of the scheduled games and attendance dropped by less than 10% compared to the preceding season, so this episode will not be considered in this paper.

It is reasonable to presume that the baseball strike should cause a reduction in personal income in MLB cities. If the typical team generates $300 million in economic activity (in 2000 dollars), the reduction in economic activity in each MLB city as a result of the strikes should be roughly $100 million in each year. Adjusting for inflation, the loss of 34% of games in 1981 should result in income losses of $53.8 million per host city. The loss of 29.5% of games in 1994 should result in a loss of $76.2 million per host city, and the loss of 11.1% of games and

29.0% of attendance in 1995 should result in the loss of $77.0 million. Of course, in Chicago and New York City, the losses should be double these figures since each city hosts two MLB teams. As a percentage of the average host city's income, i.e. personal income in the metropolitan statistical area (MSA) in which the home stadium is located, the resulting losses represent 0.151% in 1981, 0.099% in 1994, and 0.095% in 1995.

To attempt to measure the actual effect of the baseball strike, we have selected explanatory variables from past models to help establish what income would have been in the absence of the strikes and then compare these estimates to actual income levels to assess the contribution of the team to the local economy. The success of this approach depends on our ability to identify those variables that explain the majority of observed variation in growth in income in those cities that host a MLB team.

One technique is to represent a statistic for a city for a particular year as a deviation from the average value for that statistic for cohort cities for that year. Such a representation over time will, in effect, "factor out" general urban trends and developments. For example, if we identify a particular city's growth in income as 10 percent over time, but cities in general are growing by 4 percent, then we would conclude that this city's pattern deviates from the norm by 6 percent. It is the 6 percent deviation that requires explanation and not the whole 10 percent for our purposes in this study. Furthermore, if history tells us that a city that experiences a growth in income that is 5 percent below the national average both before and during a strike, then it would be misguided to attribute that 5 percent deficit to the strike. If during the strike, the city continued to exhibit income increases 5 percent below the national norm, the logical conclusion is that the residents simply substituted other spending in lieu of baseball during the strike.

Given the number and variety of variables found in regional growth models and the inconsistency of findings with regard to coefficient size and significance, criticisms of any single model could logically focus on the problems posed by omitted variables. Any critic, of course, can claim that a particular regression suffers from omitted-variable bias, it is far more challenging to address the problems posed by not including key variables in the analysis.

In explaining regional or metropolitan growth patterns, at least some of the omitted variable problem can be addressed through representing relevant variables as deviations from city norms. This leaves the scholar with a more manageable task, namely that of identifying those factors that explain city growth after accounting for the impact of those forces that generally have affected national, regional or MSA growth. For example, a variable is not needed to represent the implications of federal revenue sharing if such a change affected all cohort cities in similar ways.

Following the same logic, other independent variables should also be normalized, that is represented as a deviation from an average value for MSAs or as a fraction of the MSA average. For example, a firm's decision to locate a new factory in city i depends not on the absolute level of wages in city i, but city i's wage relative to those of all cities with whom it competes for labor and other resources. What we propose, therefore, is an equation for explaining metropolitan income growth which incorporates those variables that the literature identifies as important, but specified in such a way that those factors common to MSAs are implicitly included.

Everything discussed in this section of the paper to this point is intended to define the regression analysis that will be used to assess changes in income attributable to the 1981 and 1994 baseball strikes. Equation (1) represents the model used to predict changes in income for host cities.

$$\partial Y_t^i = \beta_0 + \beta_1 \sum_{i=1}^{n} \frac{\partial Y_t^i}{n_t} + \beta_2 \partial Y_{t-1}^i + \beta_3 W_t^i + \beta_4 T_t^i + \beta_5 BOOM/BUST_t^i + \beta_6 TR_t^i + \epsilon \qquad (1)$$

where for each time period *t*,

∂Y_t^i	= % change in income in the ith metropolitan statistical area (MSA),
n_t	= number of cities in the sample,
W_t^i	= nominal wages in the ith MSA as a percentage of the average for all cities in the sample,
T_t^i	= state and local taxes in the ith MSA as a percentage of the average for all cities in the sample,
$BOOM_t^i$	= a dummy variable for oil boom and bust cycles for selected cities and years,
TR_t^i	= annual trend,
ϵ	= stochastic error.

For the purposes of our analysis the functional form is linear in all the variables included in equation (1). This equation is calculated for each of the American MLB host cities. Toronto and Montreal are excluded due to lack of available data. While the average income variable is significant is each city's regression equation, the remaining variables specified in equation (1) are not necessarily statistically significant in each city's regression equation. In these cases, variables were removed until each remaining variable was significant at the 5% level. As is to be expected with time-series analysis, auto-correlation was identified as a problem in the regression models for most cities. Therefore, Cochrane-Orcutt regression was used in all cities to eliminate the serial correlation.

As mentioned previously, rather than specifying all the variables that may explain metropolitan growth, we attempted to simplify the task by including independent variables that are common to cities in general and the ith MSA in particular. In effect we have devised a structure that attempts to identify the extent to which the deviations from the growth path of cities in general ($\Sigma\ \partial Y_t^i / n_t$) and city i's secular growth path (∂Y^i_{t-1}) are attributable to deviations in certain costs of production (wages and taxes), demand related factors (population, real per capita personal income), and dummy variables for oil boom and bust periods as well as the region in which the MSA is located. Equation (1) was used to predict the growth path for income, and this predicted value was compared to the actual growth in income to formulate a conclusion with regard to the effect the baseball strikes on income in MLB cities in 1981, 1994, and 1995. Of course, the credibility of this procedure depends on a robust equation for predicting income growth.

The Results of Model #1

We examined the economic impact of the baseball strikes using data over the period 1969 through 2000. The time period was chosen due to the availability of city by city income data. Seventy-three cities constituted our sample, representing all MSAs that were on average the seventy-three most populous in the country over this period and includes all MSAs that appeared in the top sixty largest at any time during this period. The cities used are listed in Appendix 1 along with other information regarding the availability of data. The results of a regression for Minneapolis-St.Paul using equation (1) are represented in Table 1. While each MLB city will have different regression results, the Minnesota Twins (Minneapolis/St. Paul MSA) were used for illustrative purposes.

A brief examination of the coefficients in Table 1 reveals some interest facts about metropolitan area income growth in Minneapolis. As noted previously, not every variable is a significant predictor of income growth in every city. For Minneapolis, neither taxes, wage, nor lagged income growth is a statistically significant predictor of current income growth in the model as specified. While wages tend to be a good predictor when conducting cross-city comparisons, the rate of wage change within a city is generally too small to be a significant predictor within a city over time.

The key statistic for our purposes is the difference between the actual growth in income and that predicted for the city hosting the strike years. A complete listing of each city's expected income gains, realized income gains, and income gains above or below expected numbers is shown in Tables 2a-2c.

As shown in Table 2, in no cities in any year did the strike emerge as a statistically significant event at the 1% significance level. In thirty-six out of seventy cases, the increase in income in the MLB city was lower than expected, while income gains were above the expected amount in the remaining thirty-four cases. On average, the model predicted an increase in income in host cities of 1.950% during strike years while the observed gains in income averaged 1.976%. The strike years produced an increase in income of roughly three-hundredths of a percent above what would be expected, directly opposed to the *ex ante* estimates of decreases in income of ten to fifteen-hundredths of a percent.

The magnitude of the variation of the estimates at first blush appear high. Some host cities (New York City, 1995) exhibited billions of dollars in increased economic activity while others (Detroit, 1981; L.A., N.Y.C., 1994) experienced billions of dollars in reduced economic impact. The explanation for this range of estimates is simply that the models do not explain all the variation in estimated income, and, therefore, not all the variation can be attributable to baseball's work stoppages. The standard error of the estimates from the models indicates significant variation in the residuals from year to year for some cities. This heteroscedasticity problem is particularly apparent in cities such as Detroit, Houston, and Miami where the metropolitan economies are dominated by a cyclical industry: automobiles in Detroit, tourism in Miami, and oil in Houston. For a large, diverse metropolitan economy, even a $100 million dollar loss as a result of a MLB strike team is small portion of total annual economic activity for the area. For example, in the New York City MSA, a $100 million event would represent only 0.04 percent of the city's total GDP in 1995, a figure well below the standard error of the estimate. While a $100 million event may not appear as statistically significant in any one host city and is likely to be obscured by natural variations in the MSA's economy, one should expected that on average across the many MLB cities and numerous strike years, the average of many $100 million losses will begin to appear as statistically significant. One can observe this by adjusting the predicted income increases to assume a drop in MSA income of 34% of $300 million adjusted for inflation in 1981, 29.5% of $300 million in 1994, and 29.0% of $300 million in 1995 and standardizing the residual for each host city in each strike year. These resulting standardized errors can be used estimate p-values. Essentially, the values in the "Difference" column in Tables 2a-2c, adjusted for expected strike losses, are divided by the standard deviation of the yearly residuals for the appropriate city. The mean of these standardized residuals (= 0.13) is divided by the square root of 70 (the sample size) in order to find a t-statistic with 69 (= n-1) degrees of freedom. The residuals in Tables 2a-2c (and hence the mean of the standardized residuals) can be adjusted by assuming an economic impact larger or smaller than the booster's claims of $300 per team. The resulting p-values shown in Table 3 assume normality of the residuals. Using this methodology it is found that there is less than a 14 percent probability that the MLB teams provide $300 million or more in annual economic benefits to the host community. In fact, the data suggest that the net impact of a MLB team is approximately zero. Again it must be pointed out that due to the tiny magnitude of professional sports in the scope of an entire metropolitan economy, a 90% confidence interval for the true impact of a MLB franchise is quite large, ranging from a positive $449.6 million to negative 409.4 million.

Model #2

The key to the model presented in the previous section is to factor out experiences that are common to all MSAs in order to be able to capture the effects of the baseball strikes. This concept suggests another simple yet compelling method of calculating the impact of a MLB sports franchise on a host city. One can simply calculate the ratio of total income in MLB cities to the total income in a cohort of other metropolitan areas without MLB teams. While numerous factors such as inflation, population, the economic business cycle, expectations about the future, and seasonal variations can affect personal income in a city, most of these factors will affect personal income in other metropolitan areas in a similar manner. Therefore, although it may be difficult to predict income fluctuations in a city, if economic factors affect all cities in a given sample in the same way, then the ratio of a particular city's or group of cities' incomes to the incomes of cities in the sample as a whole should remain unchanged. If an event such as a strike significantly decreases economic activity in the host cities, then the host cities' incomes as a percentage of incomes of the cities in the rest of the sample should decrease. By comparing the

MLB city/non-MLB city ratio in a strike period to other time periods, an decrease in income can be inferred.

Several known variables will serve to shift the ratio and must be accounted for when estimating the ratio. First, if the economies of MLB cities are growing at a faster rate than the other cities in the sample, then the income ratio will grow over time. Thus, it is reasonable to include a time trend variable in the model. This time trend variable can be inserted either as a linear or a quadratic variable. The income ratio lagged any appropriate number of periods can also be included if it is a good predictor of the current period ratio. Equation (2) represents the model used to predict changes in the MLB/non MLB income ratio for our sample of metropolitan statistical areas.

$$R_t = \beta_0 + \beta_1 R_{t-1} + \beta_2 R_{t-2} + \beta_3 TR_t + \beta_4 TR_t^2 + \epsilon \qquad (2)$$

where for each time period t,

R_t = ratio of income in MLB cities to all other sample MSAs
TR_t = annual trend,
ϵ = stochastic error.

The Results of Model #2

We examined the economic impact of the baseball strikes using data over the period 1969 through 2000. As before, the time period was chosen due to the availability of city by city income data. Seventy-three cities constituted our sample, representing all MSAs that were on average the seventy-three most populous in the country over this period and includes all MLB host cities. To examine the 1981 strike, the total personal income of the 22 MSAs which hosted the 24 American MLB teams was divided by the total personal income of the remaining 51

MSAs in the sample. Ordinary least-square regression was initially attempted, but was found to have significant problems with auto-correlation as might be expected in a time-series analysis. The Cochrane-Orcutt method was used to correct for this problem, and the results of a Cochrane-Orcutt regression for equation (2) are represented in Table 4a.

An examination of the residual data for the regression model in Table 4a reveals that the actual income ratio for the MLB cities to the non-MLB baseball cities was 0.28836% higher than predicted by the model in 1981. As the personal income in the 51 non-MLB cities in the sample totaled $1.319 trillion (in 2000 dollars) in 1981, a 0.28836% increase translates into a $3.80 billion gain to MLB cities during the strike year or $158.5 million for each of the 24 American MLB teams in existence in 1981. Since the strike canceled 34% of the season, the $158.5 million gain may be assumed to represent 34% of the value of the team leading to a total impact of a MLB team on a host community of negative $466.1 million.

To examine the 1994 strike, the total personal income of the 24 MSAs which hosted the 26 American MLB teams was divided by the total personal income of the remaining 49 MSAs in the sample. The results of a Cochrane-Orcutt regression for equation (2) are represented in Table 4b.

An examination of the residual data for the regression model in Table 4b reveals that the actual income ratio for the MLB cities to the non-MLB baseball cities was 0.5491% lower than predicted by the model in 1994 and 0.1881% higher than predicted by the model in 1995. As the personal income in the 49 non-MLB cities in the sample totaled $1.880 trillion (in 2000 dollars) in 1994, a 0.5491% shortfall translates into a $10.323 billion loss to MLB cities during the strike year or $397.0 million for each of the 26 American MLB teams in existence in 1994. Since the strike canceled 29.5% of the season, the $397 million loss may be assumed to represent 29.5% of the value of the team leading to a total impact of a MLB team on a host community of $1.346 billion. The personal income in the 49 non-MLB cities in the sample totaled $1.936 trillion (in 2000 dollars) in 1995, so a 0.1881% increase translates into a $3.642 billion gain to MLB cities during the strike year or $140.1 million for each of the 26 American MLB teams in existence in 1995. Since the previous year's strike reduced attendance by 29.0%, the $5.96 million gain may be assumed to represent 29.0% of the value of the team leading to a total impact of a MLB team on a host community of negative $483.0 million.

On average, the three models predict a total impact of a MLB team on host cities of $132.3 million, or roughly one-half of the estimates provided by MLB boosters. Of course, the wide range of estimates provided here, ranging from negative $483 million to a positive $1,346 million, serves to illustrate the difficulty of estimating the impact of a small business such as a professional sports franchise on a typical large, diverse metropolitan economy.

Conclusions and Policy Implications

Major League Baseball teams have used the lure of economic riches as an incentive for cities to construct new stadiums at considerable public expense. Estimates of the economic impact of a MLB on host communities have typically been in the vicinity of $300 million. We in general would urge caution with respect to these sorts of economic impact estimates, and our analysis suggests that a figure of $300 is wildly optimistic. Our detailed city by city regression analysis over the period 1969 to 2000 reveals that cities with MLB teams actually had higher than expected income growth in the strike years of 1981, 1994 and 1995. While the range of statistically likely net economic impacts for a MLB team on a host city ranges from a positive $449.6 million to negative 409.4 million, a best guess at this impact is $16.2 million or roughly 5% of booster estimates. A second analysis of the ratio of income in MLB metropolitan areas to non-MLB metropolitan areas over the period 1969-2000, again using the strike years of 1981, 1994, and 1995 as test cases, implied that the net economic impact of a MLB team on a host city was $132.3 million. While this estimate is significantly larger than the estimate produced by the first model, the figure is still less than half that suggested by most impact studies.

Cities would be wise to view with caution the economic impact estimates provided by supporters of MLB. As a method of economic development, professional baseball, like Casey at bat, strikes out. In addition, MLB cities should worry little about potential MLB work stoppages. While baseball strikes do cause localized hardship, consumers find other outlets for their spending leaving total city income relatively untouched.

APPENDIX

Table A1: Cities and years used to estimate model in Table 1 and 2

City Name	1969 Population	1969 Rank	2000 Population	2000 Rank	Wage Data availability	Region
Akron, OH	676,214	59	695,781	77	1972-2000	Great Lakes
Albany, NY	797,010	50	876,129	68	1969-2000	Mideast
Atlanta, GA	1,742,220	16	4,144,774	9	1972-2000	Southeast
Austin, TX	382,835	88	1,263,559	47	1972-2000	Southwest
Baltimore, MD	2,072,804	12	2,557,003	18	1972-2000	Mideast
Bergen, NJ	1,354,671	26	1,374,345	44	1969-2000	Mideast
					(State data 1969-2000)	
Birmingham, AL	718,286	54	922,820	67	1970-2000	Southeast
					(State data 1970-1971)	
Boston, MA	5,182,413	4	6,067,510	4	1972-2000	New England
Buffalo, NY	1,344,024	27	1,168,552	52	1969-2000	Mideast
					(Average of cities)	
Charlotte, NC	819,691	49	1,508,050	42	1972-2000	Southeast
Chicago, IL	7,041,834	2	8,289,936	3	1972-2000	Great Lakes
Cincinnati, OH	1,431,316	21	1,649,228	34	1969-2000	Great Lakes
Cleveland, OH	2,402,527	11	2,250,096	24	1969-2000	Great Lakes
Columbus, OH	1,104,257	33	1,544,794	41	1972-2000	Great Lakes
Dallas, TX	1,576,589	18	3,541,099	10	1972-2000	Southwest
Dayton, OH	963,574	42	950,177	65	1969-2000	Great Lakes
Denver, CO	1,089,416	34	2,120,775	25	1977-2000	Rocky Mountains
Detroit, MI	4,476,558	6	4,444,693	7	1976-2000	Great Lakes
Fort Lauderdale, FL	595,651	70	1,632,071	36	1969-2000	Southeast
					(State data 1988-2000)	
Fort Worth, TX	766,903	51	1,713,122	30	1976-2000	Southwest
					(State data 1976-1983)	
Fresno, CA	449,383	79	925,883	66	1969-2000	Far West
					(State data 1982-1987)	
Grand Rapids, MI	753,936	52	1,091,986	59	1976-2000	Great Lakes
Greensboro, NC	829,797	48	1,255,125	48	1972-2000	Southeast
Greenville, SC	605,084	67	965,407	63	1969-2000	Southeast
					(State data 1969)	
Hartford, CT	1,021,033	39	1,150,619	53	1969-2000	New England
Honolulu, HI	603,438	68	875,670	69	1972-2000	Far West
Houston, TX	1,872,148	15	4,199,526	8	1972-2000	Southwest
Indianapolis, IN	1,229,904	30	1,612,538	37	1989-2000	Great Lakes
Jacksonville, FL	610,471	66	1,103,911	57	1972-2000	Southeast
					(State data 1988-2000)	
Kansas City, MO	1,365,715	25	1,781,537	28	1972-2000	Plains
Las Vegas, NV	297,628	116	1,582,679	39	1972-2000	Far West
Los Angeles, CA	6,989,910	3	9,546,597	1	1969-2000	Far West
					(State data 1982-1987)	
Louisville, KY	893,311	43	1,027,058	61	1972-2000	Southeast
Memphis, TN	848,113	45	1,138,484	54	1972-2000	Southeast
Miami, FL	1,249,884	29	2,265,208	23	1969-2000	Southeast
					(State data 1988-2000)	
Middlesex, NJ	836,616	47	1,173,533	51	1969-2000	Mideast

					(State data 1969-2000)	
Milwaukee, WI	1,395,326	23	1,501,615	43	1969-2000	Great Lakes
Minneapolis, MN	1,991,610	13	2,979,245	13	1972-2000	Plains
Monmouth, NJ	650,177	62	1,130,698	56	1969-2000	Mideast
					(State data 1969-2000)	
Nashville, TN	689,753	57	1,235,818	49	1972-2000	Southeast
Nassau, NY	2,516,514	9	2,759,245	16	1969-2000	Mideast
New Haven, CT	1,527,930	19	1,708,336	31	1969-2000	New England
					(Average of cities)	
New Orleans, LA	1,134,406	31	1,337,171	46	1972-2000	Southeast
New York, NY	9,024,022	1	9,321,820	2	1969-2000	Mideast
Newark, NJ	1,988,239	14	2,035,127	26	1969-2000	Mideast
					(State data 1969-2000)	
Norfolk, VA	1,076,672	36	1,574,204	40	1972-2000	Southeast
					(State data 1973-1996)	
Oakland, CA	1,606,461	17	2,402,553	21	1969-2000	Far West
					(State data 1969-1987)	
Oklahoma City, OK	691,473	56	1,085,282	60	1969-2000	Southwest
Orange County, CA	1,376,796	24	2,856,493	14	1969-2000	Far West
					(State data 1982-1987)	
Orlando, FL	510,189	76	1,655,966	33	1972-2000	Southeast
					(State data 1988-2000)	
Philadelphia, PA	4,829,078	5	5,104,291	5	1972-2000	Mideast
Phoenix, AZ	1,013,400	40	3,276,392	12	1972-2000	Southwest
					(State data 1972-1987)	
Pittsburgh, PA	2,683,385	8	2,356,275	22	1972-2000	Mideast
Portland, OR	1,064,099	37	1,924,591	27	1972-2000	Far West
Providence, RI	839,909	46	964,594	64	1969-2000	New England
Raleigh-Durham, NC	526,723	73	1,195,922	50	1972-2000	Southeast
Richmond, VA	673,990	60	999,325	62	1972-2000	Southeast
Riverside, CA	1,122,165	32	3,280,236	11	1969-2000	Far West
					(State data 1982-1987)	
Rochester, NY	1,005,722	41	1,098,314	58	1969-2000	Mideast
Sacramento, CA	737,534	53	1,638,474	35	1969-2000	Far West
					(State data 1982-1987)	
St. Louis, MO	2,412,381	10	2,606,023	17	1972-2000	Plains
Salt Lake City, UT	677,500	58	1,337,221	45	1972-2000	Rocky Mountains
San Antonio, TX	892,602	44	1,599,378	38	1972-2000	Southwest
San Diego, CA	1,340,989	28	2,824,809	15	1969-2000	Far West
					(State data 1982-1987)	
San Francisco, CA	1,482,030	20	1,731,716	29	1969-2000	Far West
					(State data 1982-1987)	
San Jose, CA	1,033,442	38	1,683,908	32	1972-2000	Far West
					(State data 1982-1987)	
Scranton, PA	650,418	61	623,543	84	1972-2000	Mideast
					(State data 1983-1984)	
Seattle, WA	1,430,592	22	2,418,121	19	1972-2000	Far West
					(State data 1982-2000)	
Syracuse, NY	708,325	55	731,969	73	1969-2000	Mideast
Tampa, FL	1,082,821	35	2,403,934	20	1972-2000	Southeast
					(State data 1988-2000)	
Tulsa, OK	519,537	74	804,774	71	1969-2000	Southwest
Washington, DC	3,150,087	7	4,948,213	6	1972-2000	Southeast

W. Palm Beach, FL	336,706	105	1,136,136	55	1969-2000 (State data 1988-2000)	Southeast

Complete data on population and employment was available for all cities from 1969 to 2000. This implies that data on employment growth and employment growth lagged one year was available from 1971 to 2000. Data regarding state and local taxes as a percentage of state GDP was available for all cities from 1970 to 2000, and was obtained from the Tax Foundation in Washington, D.C. Wage data from the Bureau of Labor Statistics Current Employment Statistics Survey was available for cities as described above. When city data was not available, state wage data was used in its place. When possible, the state wage data was adjusted to reflect differences between existing state wage data and existing city wage data. For MSAs that included several primary cities, the wages of the cities were averaged together to create an MSA wage as noted in Table A1.

The "Oil Bust" dummy variable was included for cities highly dependent on oil revenues including Dallas, Denver, Fort Worth, Houston, New Orleans, Oklahoma City, and Tulsa. The variable was set at a value of 1 for boom years, 1974-1976 and 1979-1981, and at -1 for the bust years, 1985-1988. While this formulation does imply that each boom and bust is of an equal magnitude, the variable does have significant explanatory value nonetheless.

Each city was placed in one of eight geographical regions as defined by the Department of Commerce. The region to which each city was assigned is shown in Table A1. Employment, income, and population data were obtained from the Regional Economic Information System at the University of Virginia which derives its data from the Department of Commerce statistics.

REFERENCES

Baade, Robert A., 1996, Professional Sports as a Catalyst for Metropolitan Economic Development, *Journal of Urban Affairs*, 18(1), 1-17.

Baade, Robert A. and Victor A. Matheson, 2000, An Assessment of the Economic Impact of the American Football Championship, the Super Bowl, on Host Communities, *Reflets et Perspectives*, 39(2-3), 35-46.

Baade, Robert A. and Victor A. Matheson, 2001 Home Run or Wild Pitch? Assessing the Economic Impact of Major League Baseball's All-Star Game. *Journal of Sports Economics*, 2(4), 307-327.

Beaudry, Charles, 2002, Why Bother? The Economic Impact of a Professional Sports Franchise, www.savetheexpos.com/impact.asp, accessed 9/5/2002.

Coates, Dennis and Brad Humphreys, 1999, The Growth Effects of Sports Franchises, Stadia, and Arenas. Journal of Policy Analysis and Management, 14(4), 601-624.

Davidson, Larry, 1999, Choice of a Proper Methodology to Measure Quantitative and Qualitative Effects of the Impact of Sport, *The Economic Impact of Sports Events*, ed. Claude Jeanrenaud (Neuchatel, Switzerland: Centre International d'Etude du Sport), 9-28.

Humphreys, Jeffrey, 1994, The Economic Impact of Hosting Super Bowl XXVIII on Georgia, *Georgia Business and Economic Conditions*, May-June, 18-21.

Munsey and Suppes, 2000, Baseball Ballparks, www.ballparks.com/baseball, accessed September 13, 2000.

Noll, Roger and Andrew Zimbalist, 1997, The Economic Impact of Sports Teams and Facilities.

Sports, Jobs and Taxes, eds. Roger Noll and Andrew Zimbalist, (Washington, D.C.: Brookings Institution).

Oregon Baseball Campaign, 2002, Why MLB, Why Oregon, Why Now? www.oregonbaseballcompaign.com/fact.htm, accessed September 5, 2002.

Porter, Philip, 1999, Mega-Sports Events as Municipal Investments: A Critique of Impact Analysis, *Sports Economics: Current Research*, eds John Fizel, Elizabeth Gustafson, and Larry Hadley (Westport, CT: Praeger Press).

Rosentraub, Mark, 1994, Sport and Downtown Development Strategy, *Journal of Urban Affairs*, 16(3), 228-239.

Saint Louis Regional Chamber and Growth Association, 2000, RCGA Estimates Economic Impact of Cardinals Playoff Run, www.stlrcga.org/00_1002.html, accessed 9/5/2002.

Seigfried, John and Andrew Zimbalist, 2000, The Economics of Sports Facilities and Their Communities, *Journal of Economic Perspectives*, 14(3), 95-114.

TABLE 1

Cochrane-Orcutt Regression Results for Income Data for Minneapolis MSA

Statistic/Value[a]	**Coefficient Values and (t-statistics)**
b_0 (constant)	-1.3641 (-4.55)*
b_1 ($\partial Y_t^i / \partial \Sigma\ \partial Y_t^i / n_t$)	0.9957 (15.06)*
b_2 ($\partial Y_t^i / \partial \partial Y^i_{t-1}$)	-
b_3 ($\partial N_t^i / \partial W_t^i$)	-
b_4 ($\partial N_t^i / \partial T_t^i$)	-
b_5 ($\partial N_t^i / \partial BOOM/BUST_t^i$)	-
b_6 ($\partial N_t^i / \partial TR_t^i$)	0.0007 (4.56)*
R^2	.9019
Adjusted R^2	.8906
F-statistic	119.37*

* Result was significant at the 99% level.

TABLE 2a

Actual vs. Predicted income growth in host cities, 1981

Year	City	Actual Growth	Pred. Growth	Difference	t-stat	Income	Income gains
1981	ANA	3.513%	3.548%	-0.035%	-0.03	$ 56,438,972	$ -19,578
1981	ATL	2.136%	3.241%	-1.105%	-0.97	$ 49,087,993	$ -542,201
1981	BAL	-0.115%	0.989%	-1.104%	-2.04	$ 50,832,822	$ -561,285
1981	BOS	1.221%	1.613%	-0.392%	-0.34	$122,032,625	$ -478,432
1981	CHC/WS	-0.572%	-0.262%	-0.310%	-0.40	$181,415,451	$ -563,025
1981	CLE	-1.281%	-1.017%	-0.264%	-0.25	$ 53,244,147	$ -140,745
1981	CIN	-0.731%	0.206%	-0.937%	-1.12	$ 31,205,137	$ -292,495
1981	DET	-4.635%	-1.884%	-2.751%	-1.54	$ 99,522,874	$ -2,738,053
1981	HOU	8.445%	6.603%	1.842%	0.76	$ 79,054,855	$ 1,456,312
1981	KC	-0.909%	0.329%	-1.238%	-1.30	$ 32,721,681	$ -404,971
1981	LA	1.351%	0.478%	0.873%	0.67	$192,451,311	$ 1,680,607
1981	MIL	-0.916%	-0.452%	-0.464%	-0.55	$ 33,295,073	$ -154,607
1981	MIN	0.467%	1.462%	-0.995%	-1.48	$ 54,596,942	$ -543,361
1981	NYM/Y	1.636%	-0.092%	1.728%	1.14	$208,286,617	$ 3,598,196
1981	OAK	1.753%	2.190%	-0.437%	-0.39	$ 47,970,341	$ -209,553
1981	PHL	0.278%	-0.007%	0.285%	0.38	$108,739,253	$ 309,665
1981	PIT	-0.297%	-0.827%	0.530%	0.63	$ 56,994,178	$ 302,119
1981	STL	0.321%	0.056%	0.265%	0.45	$ 53,879,844	$ 142,596
1981	SEA	1.635%	1.434%	0.201%	0.11	$ 44,690,981	$ 89,627
1981	SD	3.440%	3.604%	-0.164%	-0.15	$ 45,080,844	$ -73,720
1981	SF	2.814%	0.371%	2.443%	1.45	$ 50,621,535	$ 1,236,805
1981	TEX	4.351%	4.483%	-0.132%	-0.10	$ 23,457,764	$ -30,964
	Average (1981	**1.087%**	**1.185%**	**-0.098%**	**-0.23**	**$ 76,164,602**	**$ 93,770**
	Average (1994)	**2.035%**	**2.051%**	**-0.016%**	**0.08**	**$ 100,596,670**	**$ -124,943**
	Average (1995)	**2.733%**	**2.733%**	**0.184%**	**0.11**	**$ 103,375,273**	**$ 506,143**
	Average (Overall)	**1.976%**	**1.950%**	**0.027%**	**-0.01**	**$ 93,870,684**	**$ 160,167**

TABLE 2b

Actual vs. Predicted income growth in host cities, 1994

Year	City	Actual Growth	Pred. Growth	Difference	t-stat	Income	Income gains
1994	ANA	0.489%	0.070%	0.419%	0.36	$ 79,221,286	$ 331,656
1994	ATL	5.367%	5.037%	0.330%	0.29	$ 96,041,711	$ 316,612
1994	BAL	1.879%	1.089%	0.790%	1.46	$ 70,695,615	$ 558,151
1994	BOS	2.556%	1.804%	0.752%	0.66	$180,893,292	$ 1,360,779
1994	CHC/WS	2.619%	2.172%	0.447%	0.58	$241,648,864	$ 1,079,280
1994	CLE	2.106%	0.988%	1.118%	1.06	$ 63,714,982	$ 712,438
1994	CIN	2.241%	2.106%	0.135%	0.16	$ 43,137,519	$ 58,230
1994	COL	3.939%	5.262%	-1.323%	-0.86	$ 54,842,926	$ -725,394
1994	DET	4.376%	2.689%	1.687%	0.95	$128,602,419	$ 2,169,179
1994	FLA	1.279%	0.202%	1.077%	0.34	$ 48,620,281	$ 523,485
1994	HOU	2.192%	3.707%	-1.515%	-0.62	$102,378,735	$ -1,550,876
1994	KC	3.272%	2.524%	0.748%	0.78	$ 46,084,284	$ 344,934
1994	LA	-0.893%	0.131%	-1.024%	-0.78	$240,785,132	$ -2,466,451
1994	MIL	2.478%	1.913%	0.565%	0.67	$ 42,066,751	$ 237,747
1994	MIN	4.072%	3.244%	0.828%	1.23	$ 83,882,610	$ 694,897
1994	NYM/Y	0.414%	1.566%	-1.152%	-0.76	$293,618,843	$ -3,382,929
1994	OAK	0.947%	2.256%	-1.309%	-1.18	$ 69,970,299	$ -916,232
1994	PHL	0.355%	1.093%	-0.738%	-0.98	$147,523,768	$ -1,088,489
1994	PIT	0.251%	0.997%	-0.746%	-0.89	$ 64,842,032	$ -483,920
1994	STL	2.303%	1.768%	0.535%	0.91	$ 71,408,844	$ 381,987
1994	SEA	2.459%	1.756%	0.703%	0.40	$ 70,806,403	$ 498,008
1994	SD	0.443%	0.988%	-0.545%	-0.51	$ 69,532,351	$ -378,820
1994	SF	1.182%	2.785%	-1.603%	-0.95	$ 66,499,827	$ -1,065,899
1994	TEX	2.524%	3.076%	-0.552%	-0.43	$ 37,501,306	$ -207,007
	Average (1981	**1.087%**	**1.185%**	**-0.098%**	**-0.23**	**$ 76,164,602**	**$ 93,770**
	Average (1994)	**2.035%**	**2.051%**	**-0.016%**	**0.08**	**$ 100,596,670**	**$ -124,943**
	Average (1995)	**2.733%**	**2.733%**	**0.184%**	**0.11**	**$ 103,375,273**	**$ 506,143**
	Average (Overall)	**1.976%**	**1.950%**	**0.027%**	**-0.01**	**$ 93,870,684**	**$ 160,167**

TABLE 2c

Actual vs. Predicted income growth in host cities, 1995

Year	City	Actual Growth	Pred. Growth	Difference	t-stat	Income	Income gains
1995	ANA	1.812%	1.631%	0.181%	0.16	$ 80,656,411	$ 145,617
1995	ATL	5.519%	5.327%	0.192%	0.17	$101,342,437	$ 194,771
1995	BAL	1.329%	1.314%	0.015%	0.03	$ 71,634,904	$ 10,486
1995	BOS	2.457%	2.945%	-0.488%	-0.43	$185,337,596	$ -904,698
1995	CHC/WS	3.647%	2.713%	0.934%	1.21	$250,460,769	$ 2,338,237
1995	CLE	1.219%	1.517%	-0.298%	-0.28	$ 64,491,874	$ -191,977
1995	CIN	1.464%	2.359%	-0.895%	-1.07	$ 43,769,041	$ -391,744
1995	COL	5.568%	4.217%	1.351%	0.88	$ 57,896,381	$ 781,970
1995	DET	2.707%	2.991%	-0.284%	-0.16	$132,083,299	$ -375,514
1995	FLA	3.164%	2.711%	0.453%	0.14	$ 50,158,671	$ 227,265
1995	HOU	4.753%	3.808%	0.945%	0.39	$107,245,066	$ 1,013,748
1995	KC	2.529%	2.993%	-0.464%	-0.49	$ 47,249,955	$ -219,035
1995	LA	1.247%	0.767%	0.480%	0.37	$243,788,401	$ 1,170,871
1995	MIL	1.818%	2.171%	-0.353%	-0.42	$ 42,831,638	$ 151,080
1995	MIN	3.615%	3.695%	-0.080%	-0.12	$ 86,915,313	$ -69,173
1995	NYM/Y	3.402%	0.710%	2.692%	1.78	$303,606,418	$ 8,171,701
1995	OAK	2.863%	2.907%	-0.044%	-0.04	$ 71,973,729	$ -31,483
1995	PHL	1.456%	1.150%	0.306%	0.41	$149,671,812	$ 458,095
1995	PIT	0.793%	0.890%	-0.097%	-0.12	$ 65,355,969	$ 63,658
1995	STL	2.263%	1.753%	0.510%	0.87	$ 73,025,146	$ 372,755
1995	SEA	2.657%	3.401%	-0.744%	-0.42	$ 72,687,993	$ -540,528
1995	SD	1.292%	1.719%	-0.427%	-0.40	$ 70,430,368	$ -301,083
1995	SF	4.772%	3.799%	0.973%	0.58	$ 69,672,963	$ 677,671
1995	TEX	3.251%	3.705%	-0.454%	-0.35	$ 38,720,405	$ -175,791
	Average (1981	**1.087%**	**1.185%**	**-0.098%**	**-0.23**	**$ 76,164,602**	**$ 93,770**
	Average (1994)	**2.035%**	**2.051%**	**-0.016%**	**0.08**	**$ 100,596,670**	**$ -124,943**
	Average (1995)	**2.733%**	**2.733%**	**0.184%**	**0.11**	**$ 103,375,273**	**$ 506,143**
	Average (Overall)	**1.976%**	**1.950%**	**0.027%**	**-0.01**	**$ 93,870,684**	**$ 160,167**

TABLE 3

Probabilities for Various Levels of Economic Impact Induced by a MLB Team

Economic Impact	Probability of such an impact or greater having occurred
$441.6 million	5.00%
$346.3 million	10.00%
$300 million	13.49%
$200 million	23.68%
$100 million	37.16%
$0	52.50%
negative	47.50%

TABLE 4a

Cochrane-Orcutt Regression Results for Income Ratio Data for 1981 strike

FINAL PARAMETERS:

Estimate of Autocorrelation Coefficient	
Rho:	-0.7029
Standard Error of Rho:	0.14520

Cochrane-Orcutt Estimates

Multiple R	0.9995
R-Squared	0.9990
Adjusted R-Squared	0.9987
Standard Error	0.0046
Durbin-Watson	2.1512

Analysis of Variance:

	DF	Sum of Squares	Mean Square
Regression	4	.47241	.11810
Residuals	23	.00049	.00002

Variables in the Equation:

	B	SEB	BETA	T	SIG T
$Ratio_{t-1}$	1.39348	.10479	1.48841	13.2982	.00000
$Ratio_{t-2}$	-.74493	.09083	-.85725	-8.2014	.00000
Time	-.00663	.00127	-.73948	-5.2296	.00003
$Time^2$	.00010	.00002	.39059	5.1683	.00003
Constant	.51908	.09893		5.2472	.00003

TABLE 4b

Cochrane-Orcutt Regression Results for Income Ratio Data for 1994 strike

FINAL PARAMETERS:

Estimate of Autocorrelation Coefficient

Rho:	-0.5833
Standard Error of Rho:	0.16580

Cochrane-Orcutt Estimates

Multiple R	0.9995
R-Squared	0.9990
Adjusted R-Squared	0.9988
Standard Error	0.0046
Durbin-Watson	2.2484

Analysis of Variance:

	DF	Sum of Squares	Mean Square
Regression	4	.47531	.11883
Residuals	23	.00048	.00002

Variables in the Equation:

	B	SEB	BETA	T	SIG T
$Ratio_{t-1}$	1.39664	.11815	1.48051	11.8205	.00000
$Ratio_{t-2}$	-.73423	.11176	-.82991	-6.5697	.00000
Time	-.00671	.00160	-.69355	-4.2012	.00034
$Time^2$	.00010	.00002	.36184	4.3833	.00022
Constant	.54961	.13312		4.1019	.00044